Introducing Students to Research by Use of Biographical Materials in a Comparative Psychology Course

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Students in this course learn not only the major content of the subject, but also the psychology of becoming a major psychologist.

In a college basketball game in 1979, one team used extreme stall tactics for the first 20 minutes and the halftime score was 7-0. The next day a sportswriter for the *Raleigh Times* said that the first half was about "as exciting as scientific research." To outsiders, scientific research must seem about as exciting as a 7-0 basketball game.

Undergraduate students are generally taught the results of research, and techniques for doing research, but not what it is like to be the researcher. They learn about discoveries, but not so much about the process of discovering. Only seldom do they hear anecdotes about the lives of noted researchers—though they generally remember the ones they do hear about (see McConnell, 1978). Thus the student is apt to have difficulty identifying with researchers, or understanding the pleasure and excitement of scientific work.

Even a graduate student usually learns about academic or scientific life only through a few examples, such as that of the student's advisor. A young person entering a non-academic, non-scientific career would, in contrast, probably give close examination to the careers of others who have achieved success in the field. One can hardly imagine an aspiring politician who would not study other politicians' routes to success, or a young movie director who would not compare the styles of various directors of the past. We tend to present science, however, as a less personal field, in which individual differences among investigators are either uninteresting or unimportant.

For the last few years I have been teaching a comparative psychology (i.e., animal behavior) course for graduate and advanced undergraduate students, in which we organize the field not by one behavior at a time, or by species, or by theoretical issue, but in terms of one investigator at a time—ordinarily, one per week. We discuss that investigator's research findings, and the possible applications or theoretical importance of those findings, as one would in any other course. However, we also discuss anything we can learn or infer about the investigator's motives, scientific style, and personal life, and the relationship of these to the research. We consider, for instance, the following questions:

(1) Why did the investigator choose this topic for investigation and approach it this way?

(2) Why did the investigator change research interests, or (in other cases), why did this person stay with one topic for such a long time? In general, how can one decide when a research area has reached a point of diminishing returns?

(3) What led to this individual's success? What, if anything, do successful investigators have in common? (The latter question is problematical, because the investigators discussed are not necessarily a representative sample of all successful investigators. However, because these investigators were selected with the intention of having as much diversity as possible (see below), any patterns which emerge may be of interest.)

(4) For what kind of student would this investigator be a good "role model"?

The Investigators selected for Study. During a semester we deal with 14 investigators, including eleven "regulars" almost always included, and others selected by the students from a list of suggestions. The eleven "regulars" were chosen for a variety of reasons, with no effort to choose the eleven greatest investigators. I selected individuals who were alive (and therefore capable of responding to requests for information), interesting, and clearly successful in research (though not necessarily famous). Beyond that, I sought individuals involved in a large sample of the major research fields of comparative psychology, such as animal learning, animal social and sexual behavior, invertebrate behavior, ethology, and brain mechanisms. Finally, I wanted investigators with as great a variety of research "styles" as possible, including such dimensions as long-term persistence with one topic versus repeated changing of interests; theory testing versus phenomenon demonstration; laboratory research versus library research; global overview versus emphasis on detail; and bold gambles versus a cautious, step-by-step approach. I included one investigator, John Money, who is not really a comparative psychologist in any usual sense of the term, because he represents clinical research, which is important to contrast against laboratory research, and because his work on human sexual behavior has interesting overlaps and parallels with some areas of comparative psychology.

For the sake of diversity, I included four investigators from outside the United States (Hinde, Mackintosh, Rensch, and Tinbergen). I would have liked to include one or more women, but there have not been many female comparative psychologists until very recently. However, although there are no women among the eleven "regulars," I include several among the suggestions from which we select additional people for a given semester. A list of the eleven "regulars" follows (alphabetically), with a brief description of each:

Frank Beach. Area: Hormones and behavior, mainly with regard to animal sexual behavior. Style: Pioneered a previously almost unknown area; investigated a long series of related questions.

Harry Harlow. Area: Social behavior and complex learning and reasoning in monkeys. Style: Intensive study of a small number of individuals over time. Attempt to relate animal studies to human problems in education and psychopathology.

Robert Hinde. Area: Social behavior in birds and monkeys. Style: Extensive scholarship and data-collection. Very cautious about drawing broad, general conclusions. Has also made a major contribution through textbooks and comprehensive review articles.

N. J. Mackintosh. Area: Role of attention in animal learning; use of species comparisons to elucidate mechanisms of behavior. Style: Long series of studies, frequently complex in design, on a single topic. Working in a crowded research area, often reacting to others' work, often dealing with detailed issues derived from previous research.

John Money. Main area: Human sexual behavior, especially abnormalities (with some attempts to relate human data to the literature on animals). Style: Clinical rather than laboratory studies. Forced to deal with syndromes and phenomena as they come to his attention, rather than deciding in advance on the number and age of subjects, etc.

Bernhard Rensch. Main area: Evolution of intelligence, and its philosophical implications. Many other projects dealing with precursors to aesthetics in nonhumans, aesthetics in humans, descriptive ethology, brain physiology, and other topics. Style: Worked for many years as a curator of molluscs at a museum, publishing on systematics and evolution. Later began work on a great many aspects of comparative psychology. Still later, attempts to synthesize his apparently diverse contributions into a grand philosophical framework, focusing on the mind-body issue.

Curt Richter. Area: Biological rhythms, food selection, GSR, behavioral effects of endocrine abnormalities. Style: A pioneer in unexplored areas, a master of serendipity. More involved with collecting data and demonstrating phenomena than testing theories. Worked largely on his own, with relatively few students.

Kenneth Roeder. Area: Neurophysiology of ecologically significant behaviors in insects. A pioneer of "neuroethology." Style: Long series of studies on a few closely-related issues.

Paul Rozin. Area: Food-selection in animals and humans, reading acquisition, the evolution of intelligence, and miscellaneous. Style: Several major changes of research focus. Likes unexplored areas; likes to gain some insight into a mystery, rather than to work on highly detailed questions.

Larry Stein. Area: Pharmacology of self-stimulation of the brain in rats. Style: Empirical work stays within narrow confines, but bold theories stretch out in many directions. Proposed animal models for manic-depressive psychosis, schizophrenia, anxiety, repression, etc.

Niko Tinbergen. Area: Ethology (one of its founders). Style: Observations of animals in the wild, with conceptually sophisticated yet methodologically simple experiments. Good balance of data and theory. In later years, suddenly turned attentions to childhood autism.

Design of the Course. The mechanics of the course have not been unusual. Typically I take charge of lectures and discussion for the first few weeks of the semester, and students give presentations later in the semester. That is, a student would be in charge of reading the major publications and any available biographical materials for a given investigator. Occasionally students have directly contacted their investigators, by letter or phone, once even in person, to get additional information. One investigator once came to class in person.

To get materials for the course, I wrote to each of the eleven "regulars" and several others and obtained vitae, reprints, and sometimes written biographical materials. Ten of the elevel "regulars" provided a specially-made tape-recorded autobiographical monologue, or a tape-recorded interview with me. (The level of cooperation was surprisingly high. Evidently the compliment of being included in a course on "great biological psychologists" was highly effective.)

Some published biographical materials are also available, such as Beach's autobiography (1974) and Rozin's (1976) biographical article about Richter. All of these materials are made available to the students; the tape recordings are played in class.

Insights Gained From This Course. This course was designed for instructional purposes, not as systematic research into the psychology of great scientists, as Roe's (1953) study was, for example. However, this group of investigators, intentionally selected for diversity, shows some interesting patterns of overlap. A series of classes over several years has formed some tentative conclusions about what factors were important for research success, at least for this group of investigators. Those conclusions are discussed below.

Investigators' choice of research topic. The choice of a research topic is obviously one of the most important decisions any investigator makes, if not *the* most important. However, it is difficult to form good rules for making this decision. When I asked the various investigators to provide me with a tape-recording or an interview, one of the questions I suggested for discussion was how one decides which experiment to do, or when to change interests. Very curiously, none had much insight into this important aspect of their own behavior. In many cases, the original choice of a research topic, and any later changes, seemed as much a matter of circumstances and accident as of any deliberate plan. The closest thing to a self-insight in this regard was Rozin's statement that he prefers "uncrowded" areas:

> I tend to move into vacuums . . . . It seems to me that the distribution of psychologists across problems is both far from random and far from optimal. There tend to be bandwagons, problems that become fads, and large numbers of people go into them. My strategy has been to gravitate toward fields which seem to have great promise and nobody in them. And that makes it easy to do rather wide-open research and not to be hounded on all sides by people doing about the same thing.

Perhaps a more difficult decision than the initial research area is any later change in research direction. Virtually any research area eventually reaches a point of diminishing returns, at which it is no longer fruitful to pursue the same questions. But it is not always easy to determine when one has reached that point. Different people deal with this problem in different ways. N. J. Mackintosh has pursued a long series of experiments on basically a single set of questions. Curt Richter investigated a number of topics, all related to the common theme of "spontaneous behavior;" several times he shifted interest from one set of questions to another, and sometimes he returned to a topic after a delay of several years. Paul Rozin and Bernhard Rensch have, at times, conducted several unrelated or loosely related research activities at the same time.

Investigators' motivations for research. The individuals we studied had a variety of different motives for doing research. The only common theme was that their goals were more theoretical than applied. Consider the following quotation from Kenneth Roeder:

> I study the nervous systems of insects because I believe this offers the best opportunity for understanding how behavior is generated .... We know how individual neurons work in large part; we know something about how they communicate with each other. How can we use this information to discover how the whole population operates? That's a central question, and it seems to me that one of the best ways to answer it is to choose a simple population, such as the population in the nervous system of a moth .... Now, that's a public-relations answer. There's another way in which the question can be put, and that is, "Why do you spend your time messing around with moths when there is so much of importance that needs to be done?" And of course I can answer the question just about as rudely by simply saying, "I do it because I like it, simply because I am curious about these things."

Or from Frank Beach:

> To what degree should my choice of research work be governed by human needs, by social imperatives, and how am I going to justify spending all of my energies on any research that does not bear directly on pressing human problems? .... The solution, or rationalization, that I have finally come up with is that it is a perfectly worthwhile way of spending one's life to do your level best to increase human knowledge, and it is not necessary nor is it always even desirable to be constrained by possible applicability of what you find to immediate problems.

Bernhard Rensch says:

> [My main aim was] the understanding of the gradual phylogenetic development of all psychic phenomena which finally led to human abilities. It was my principal aim to get a broad experimental basis for statements and speculations about the development of sensations, mental images, and thought processes in the course of evolution. This knowledge was important for my philosophical studies, which I pursued since my student days.

And finally from N. J. Mackintosh:

> "I suppose that I probably tend to answer questions that have been posed for me by my reading of current literature rather than dreaming up wholly new questions .... It seems to me that a reasonably critical reading of any set of experiments or theoretical statements should always suggest specific questions that have not been answered .... Experiments are worth doing only to the extent that they advance our understanding of theoretically significant psychological processes."

This emphasis on theory rather than application is probably more characteristic of comparative psychology than of some other research areas. It will be interesting to see whether the same emphasis will be characteristic of the coming generation of comparative psychologists, given current presures toward relevance and application.

What made these scientists more successful or more noteworthy than most others? Seven general trends seem to characterize a majority of the investigators we studied. Again, it is not clear whether these trends are of general validity or whether they represent a sampling bias. The latter may be more involved for some factors (especially #1 and #3) than for others.

(1) Pioneers in new research areas. Of the individuals selected for study, only Mackintosh was clearly in a crowded research area, with a number of competing investigators. Some, such as Beach, were pioneers in areas that later became crowded. Others, such as Roeder, worked on topics that have never been popular research areas. A research area of broad interest is also an obvious key to achieving wide recognition—as witness the examples of Money and Harlow.

(2) Much time for research. Beach, Hinde, Mackintosh, Money, Richter, Stein, and Tinbergen were all, during at least part of their careers, in virtually pure-research jobs. Rensch is the only clear exception in this regard, being constrained by the economic and social problems of Germany for most of his career, and being forced for years to limit his research and writing to the weekends.

(3) Simple experimental designs. There are some exceptions in this regard, especially in Mackintosh's work, but generally these individuals were working on simple, fundamental problems that did not require complicated research designs. This factor is probably more characterictic of comparative psychologists than of leaders in most other parts of psychology. On the other hand, consider the following question, which I sometimes pose to students: What did Freud, Piaget, Skinner, Pavlov, Sherrington, and Richter have in common? The answer is that each of them seldom if ever published any statistic more complicated than a mean.

(4) Enjoyment of actually doing the research. Great researchers seem to enjoy being in the lab; virtually everyone who sent me any autobiographical materials spontaneously volunteered something about really enjoying not just the data and the theories but the actual research process. The quote from Roeder, above, is one example. Curt Richter has repeatedly said that he enjoys doing research more than eating. That must be true, since he has continued collecting data well beyond the emeritus age. In fact, considering the enormous volumes of data he has collected, it is hard to imagine when he finds time to eat.

Tinbergen, In *Hering Gull's World* and other publications, has remarked on the ecstasy of watching animals in nature. And in the words of yet another great student of animal behavior, Konrad Lorenz, "If you want to learn everything there is to know about ducks, you had better like ducks."

(5) Research independence at an early age. Richter, Rozin, Tinbergen, Money, and Beach all did PhD dissertations largely on their own, without close direction from their advisors. Roeder, who did not formally complete a PhD, was always on his own. Each of the others branched out in new

directions early in their careers. (It is curious that graduate students are usually told to follow in their advisors' footsteps, though few of the greatest scientists followed that pattern.)

(6) Research career could not be extrapolated from early post-PhD years. Richter's first seven publications dealt with seven quite independent topics, only two of which were major concerns of his later research. Similarly, most of the others are best known for accomplishments quite different from the topics of their first few publications.

(7) Finally, one intangible that is hard to describe. Call it "lucky guesses," if you wish, but several of these investigators obtained some exciting, "unexpected" results from experiments that would hardly have been worth doing if they had not produced the "unexpected" result. For instance, Curt Richter once did some potentially very boring experiments on swimming ability in rats, when he discovered the now-famous "sudden death" phenomenon. From that and other incidents, one can say that Richter had a great talent for serendipity, that is, for appreciating the exciting unexpected finding that someone else might have ignored.

In some other cases, however, a talent for serendipity doesn't seem to be the whole story. Experiments were done which led to a new theory or a new way of looking at a problem, but the experiment wouldn't have made any sense *without* that new theory or viewpoint. That is, great scientists sometimes see the overall pattern before they have many facts (a point previously made by Polanyi, Jung, and others).

In Larry Stein's words:

> I can't say this very well; I've been trying a couple of times lately . . . . It seems like there are some maps or tables of organization in the brain. There may not be that many ways things can be organized or relate to each other. And it may be that in order to understand our world we may have evolved these little maps or structures in the brain which we use as models as to how the world is organized, that through evolution those creatures that had the best maps had the best chance of survival. (It's hard to say it any better, because it does seem to me a very far-fetched concept.) Everybody has [these structures] because they come with the genes, but different people, the creative people, have better access to them, or trust them more, than other people. And so you confront certain problems, things that you want to explain, either in your science or in daily life, and you form pictures of the world, based on hints from our environment, comparing again these images we have in our heads as to the way things are organized, and so we refine them somewhat or we hunt around for a better one if we have an assortment of models to try and compare. And I think we're in an active process, particularly those who are actively creative in science, in an active process of finding the best model, the best picture, for the phenomenon we're trying to understand. And so since you're kind of doing this all the time, then when a paper appears or someone tells you something, or you go to a meeting and hear something, that resolves a difficulty or that is a missing link in the puzzle, you can recognize it almost immediately . . . . I think the way some good scientists do it is, they trust their feelings.

Conclusions. I have concentrated mostly on the content of the course, and what the students and I have learned from it, rather than on instructional methods. The reason is simply that the content is the innovation of the course.

A formal evaluation of the success of any small graduate-level seminar is necessarily difficult; I can merely list the goals that we seem to accomplish by organizing comparative psychology in terms of the work of a limited number of investigators:

(1) Because the investigators who were studied span a wide range of research topics, the course acquaints the student with a fair amount of the field of comparative psychology. Some important topics are omitted, I concede, but in compensation, the students learn about some exciting old experiments by Richter, Rensch, Harlow, and others which for various reasons never found their way into the textbooks.

(2) Students become familiar with a variety of role models, and learn that there are a variety of ways to make successful contributions to science.

(3) Students contemplating a possible research career are forced to consider the career decisions faced by scientists, and the purposes and motivations behind science. They also get some feeling for the personal qualities and the environmental opportunities that are necessary for success.

(4) Finally, students begin to see science as a very human enterprise, influenced by the investigators' personalities, and (usually) more exciting than a 7-0 basketball game.

A course of the same type could be organized for other topics, just as easily as for comparative psychology. The advance preparation is clearly greater than for most other courses, but not prohibitively so. It has been my experience that most of our subjects have been highly cooperative about providing reprints and biographical materials for use in a course of this type. It would also be possible to base a course on investigators for whom published biographies and autobiographies are available, in such sources as Krawiec (1972, 1974) or the *History of Psychology in Autobiography* series.

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Note

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